lengths can be readily discovered; these respond well to the forces, and their tides must be the ruling semi-diurnal tides of the oceans. The charts prove the existence of large stationary oscillations. To doubt this fact would be scarcely more reasonable than to doubt the existence of the tide itself. The large ranges of tide imply critical lengths, and critical lengths imply that the phase is controlled by the resistance to the movement.

In a second approximation it may be possible to take into account the actual departures from critical lengths, and to make some numerical estimates of the resistance, and to fix more accurately the modes of oscillation having regard to the deflecting force of the earth's rotation. In my paper the latter effect has been considered only in reference to arms or bodies of water tidally dependent upon larger bodies.

As soon as my critics develop their tidal theories sufficiently far for making definite suggestions, I shall be pleased and bound to give such developments careful consideration. In the meantime, I believe that nothing is gained by criticism which does not constantly revert to such facts as have been brought out through observations upon the tides. These constitute the final test of all theories.   R. A. HARRIS.

Washington, D.C., January 26.

It is Mr. Harris's theory with which we were, and continue to be, at variance. We were unable to gather the part played by this theory in the construction of his series of cotidal charts, and hence our statement that this construction was but "vaguely indicated"; but we are glad to be assured that the theory has only been employed in regions where observational data were entirely wanting, and has not been allowed to vitiate, as we feared, results obtained direct from observation.

In reference to the phase theorem which we selected for special comment, Mr. Harris now states that "the large ranges of tide imply critical lengths, and critical lengths imply that the phase is controlled by resistance." The latter part of this theorem we are not prepared to admit unless it be further contended that the critical conditions implied are mathematically *exact,* especially in consideration of the comparatively small frictional influences which can be brought to bear on the motions of the sea. Any departure from the ideal critical state, and we contend that such departures must inevitably occur in a complex system like that of the ocean, will render the determination of phase dependent on such departures as well as on frictional influences, and we differ from Mr. Harris in regarding the former rather than the latter as the more powerful controlling influence in regard to phase. Whence can the large resistances to motion, implied in Mr. Harris's theory, arise?   S. S. H.

Atomic Disintegration.

ACCORDING to the investigations on radium, especially by Prof. Rutherford, there can be no longer any doubt that the formation of helium from radium is due to spontaneous disintegration of the radium atom, and it is the same with the other radio-active elements. Most competent investigators have not hesitated to apply the same point of view also to all the other elements.

The enormous amount of energy set free in the formation of helium—about 10^9 great calories for a gram-atom of helium—must render hopeless any attempts to reverse this process. Considering the conformity of the other gases of the helium type—neon, argon, krypton, and xenon—it seems probable that they owe their existence to a similar disintegration of atoms. It is not surprising, therefore, that all attempts have failed to obtain a chemical compound of those gases, and I do not think such attempts likely to succeed in future. That, as yet, those gases, excepting helium itself, have not been recognised as products of atomic disintegration may be due to their difficult test.

Now it seems to me there is nothing contrary to the view that *disintegration of atoms is an irreversible process, strictly analogous to dissipation of heat.*

Considered in this way, there exists a parallelism not only as regards the first law of thermodynamics—conserv-ation of energy—with the principle of conservation of matter, but also regarding the second law—dissipation of energy, on the one hand, and atomic disintegration on the other. And as it has been stated by Clausius that the world's entropy tends towards a maximum, we may say that likewise the quantity of free helium and the similar "Edelgase" tends towards a maximum.

This parallelism in material and energetical law appears to me well worthy of notice.   W. MEIGEN.

Freiburg i/Br.

Phosphorescence of Pyro-soda Developer.

SOME time ago (January, 1904) you were good enough to publish a note on the "Phosphorescence of Photographic Plates," and the following additional particulars of this phenomenon may be of interest. The developer used is the ordinary pyro-metol-soda solution.

If a bromide plate is exposed in the camera, developed, washed for a few moments only, and then placed in aluminium sulphate solution in the dark, the picture becomes luminous and shows forth as a *negative,* the high lights being dark, whilst the shadows are bright, the darkest ones phosphorescing most strongly. If, however, the plate (after having been exposed and developed) is washed thoroughly for half an hour by means of a jet of water under pressure, no phosphorescence is observed on treating it with Al_23SO_4 solution, from which it appears that a trace of the developing solution is necessary to cause phosphorescence in the plate.

If a few spots of unused developing solution are placed in the bottom of a porcelain dish and Al_23SO_4 solution is added (in the dark), the mixture will phosphoresce. But if the developer has been used for developing exposed plates, then its power of phosphorescence is weakened, and if the same portion of solution is used repeatedly for developing, and tested periodically for phosphorescence between the developments, it will be found that its phosphorescing power is diminished after each development, and that it finally vanishes altogether. This explains the production of the phosphorescing negative. The most strongly lighted part of the film is that which will destroy the phosphorescing power of the developer it has absorbed, and the unlighted portion or shadow is that in which the absorbed developer will be least changed, and therefore most strongly phosphorescent.

The addition of various substances to the aluminium salt modifies its phosphorescing power, and some prevent it altogether, even when added in very small quantities. Among those substances which strongly counteract the phosphorescence may be mentioned the salts of thorium, uranium, copper, lead, bismuth, iron, tin, cobalt, nickel, chromium, zinc, cadmium, mercury, platinum, and silver in the order named, while the salts of potassium, sodium, ammonium, lithium, calcium, barium, strontium, magnesium, and manganese seem to have little influence one way or the other.

The only substance found which has the effect of much increasing the brilliancy of the phosphorescence is gold. A solution of $AuCl_3$ alone, in fact, gives a more brilliant phosphorescence than Al_23SO_4. The gold is reduced to the black metallic form, and while this reduction is proceeding light is emitted. Other reducing agents, however, do not appear to emit any light during the process of reduction of $AuCl_3$. The influence of the other metals on the phosphorescing power of the gold solution seems to be practically the same as for aluminium.

Other aluminium salts, such as the nitrate, phosphate (dissolved in HCl), chloride, &c., phosphoresce with pyro-metol developer, but none so brilliantly as the sulphate.

                                                T. A. VAUGHTON.

Ley Hill House, Sutton Coldfield, February 13.

Emission of Light by Kanal-strahlen Normal to their Direction.

IN a former publication (*Physik. Zeitschrift,* vi., 892, 1905) I have proved that the stream of positive ions which form the Kanal-strahlen show the Doppler effect. In these rays we have, therefore, a positive charge, and at the same time velocity, and also, as a result of the vibrations of the negative electrons, emission of light. Therefore it is

exerting a pressure on the ions against the translation resulting from radiation; besides this force an electromagnetic force—of second order of the ratio of velocity of translation to velocity of light—may arise from the moved charges of the ions and act on the vibrating electrons. The experimental research of the light of Kanal-strahlen emitted normally to their direction has given the following results. The observations have been made on hydrogen; the velocity of the Kanal-strahlen was $0.9.10^8$ and $1.2.10^8$ cm. sec.$^{-1}$. The spectrograms were taken with a prism-spectrograph and with a concave grating of 1 metre radius.

The total radiation of the line spectrum ($H\alpha$, $H\beta$, . . .) is partially polarised, and the electrical vibrations parallel to the direction of translation have a greater intensity than the vibrations at right-angles to the direction of translation. The difference of intensities is very small.

The lines of hydrogen (when observed normal to the Kanal-strahlen) are displaced towards the red, when compared with the lines emitted by the slow ions in the negative glow. The displacement seems to be proportional to the wave-length, and also proportional to the square of velocity. The displacement of the centre of $H\beta$ is approximately 0.8 Ångström unit for a velocity of $1.2.10^8$ cm. sec.$^{-1}$.

Besides this displacement there is observed a broadening of the lines; it seems also to be proportional to the square of velocity, and to increase somewhat with decrease of wave-length. The observations as to the splitting up into components of the broadened line, and also as to the polarisation of its edges, are not concordant enough in the different spectrograms, and are therefore not ready for publication. J. STARK.

Göttingen, January 6.

Inversion-point of the Joule-Kelvin Effect.

IN discussing the Joule-Kelvin effect for a fluid like hydrogen, which shows an inversion point above which heating takes place on free expansion, it is usually assumed that this point is unique. Thus, for example, Olszewski has fixed it experimentally at $-80°.5$ C. An examination of the consequences of any of the usually assumed equations of state (such as Van der Waals's or Dieterici's) easily reveals the fact that it must in reality be a function of the pressures to which the gas is subjected. But this is not all. If these consequences are examined for the inversion point corresponding to an infinitesimal change in pressure, it is seen that all the equations of state (which at the same time indicate a critical point) demand that there shall be *two* inversion points (if any) for any given pressure, and that, moreover, for sufficiently high pressures no inversion point will exist. Different equations of state, while unanimous in the above respects, indicate very different temperatures at which inversion should occur. I desire to point out, therefore, that a complete determination of the inversion points corresponding to various pressures affords an exceedingly sensitive means of discriminating between characteristic equations and of indicating the direction in which these require modification.

This matter is discussed in detail in a paper shortly to be published. ALFRED W. PORTER.

University College, W.C., February 19.

A Definition of Temperature.

A BODY containing heat is in a condition from which it tends to release itself (by radiating or conducting away heat), and this tendency only ceases when the body has passed into a heatless condition. The temperature of a body is the *measure* of its tendency at any instant to recover this heatless state (*cf.* Maxwell, "Theory of Heat," 10th ed., p. 32). This suggests a mechanical analogy; a body containing heat is analogous to an elastic medium in a state of strain, from which it tends to release itself in virtue of its restitutional forces; the magnitude of the restitutional force when a body is in a given strained condition measures its tendency to release itself from that strain, and so is analogous to the temperature of a body when in a given thermal condition. The quantity of work

stored up in producing this strained condition, and which can be given out again when the body returns to its unstrained condition, is analogous to the quantity of heat the body contains when at a given temperature; it is quite easy to show that we can completely represent the thermal condition of a body by means of a model consisting merely of an elastic rod subjected to a tension. *A temperature, therefore, is analogous to a tension or pressure.* We are now in a position to give a real physical meaning to the " temperature " of a body, and so enable it to be measured in *absolute* units like a mass or a length. Let us take a molecular body devoid of all heat motion and plunge it into a medium the temperature of which is T. Then the medium will exert an intermittent pressure or force on the molecules, thus setting them into motion and generating heat motion in the body. It can easily be shown that this force cannot be infinite, or a cold body placed in a hot medium would *instantly* acquire the temperature of the medium, whereas it always takes a definite time to do so.

The *maximum force* which the medium exerts on a molecule at rest when placed therein is the numerical value of its temperature. Hence we arrive at the following definition of temperature :—

A molecule at rest when placed in a medium possessing temperature is subjected to an intermittent pressure; the greatest value of this pressure is the correct measure of the temperature of the medium in the neighbourhood of the molecule. Another method of stating the same thing is to say that the greatest force required to hold a molecule at rest when placed in a medium is the measure of the temperature of the medium. Still another statement is to say that the temperature of a medium is the magnitude of the force tending to drive heat motion into an absolutely cold body placed therein. A temperature, therefore, should be measured as a pressure in *dynes* per sq. cm. All the ordinary laws of thermodynamics, the flowing of heat from bodies of higher to bodies of lower temperature, Waterston's hypothesis, &c., follow quite simply as a consequence of this definition, as the reader can doubtless work out for himself. GEOFFREY MARTIN.

Kiel, February 10.

Chinese Names of Colours.

IN reply to the letter of Mr. Alfred H. Crook contained in your issue of January 11, I would say that it is possible that the explanation of the Chinese colour-name is to be found in the violet coloured halo which is very commonly noticed by Alpine climbers surrounding moving objects. Dr. Ellis attributes it, I believe, to fatigue of the eye (see discussion in NATURE, May, 1897).

REGINALD A. FESSENDEN.

IN your issue of January 11, Mr. Alfred H. Crook, of Hong Kong, asks why the Chinese should call a bright purple (almost a mauve) " snow green," and he adds that the term " green " is sometimes applied to the colour of the sky, which I take to mean blue. The following is a possible explanation :—

One of the commonest places in nature to find purplish hues is in shadows, and shadows on the snow, when the sky is clear, are decidedly purple. If purple is to be classified among the colours, it will go with the blues, hence " snow green " as meaning " snow blue " would not be such a misnomer as might at first sight appear.

Pittsburg, Pa., February 7. ALFRED SANG.

Sounding Stones.

MR. ALFRED TINGLE (January 4, p. 222) and Mr. Carus-Wilson (January 11, p. 246) may be interested to know that at the caves of Ellora, near Aurangabad, one of the pillars in the rock-cut temples has the same property of sounding under a blow.

The pillar is a massive one close to, or part of, the doorway leading to an inner shrine, and if struck with the clenched fist emits a deep note.

So far as I recollect, this property was confined to a portion of the pillar. W. G. BARNETT.

Poona, January 29.